

relations of the beds. Another most valuable portion is the series of tables of localities where the different formations may now be studied, showing no less than 154 places worthy of a geological visit within twenty miles of London. With regard to the general structure of the district, Mr. Whitaker is careful to refute the idea that the Tertiary beds were deposited in an eroded hollow of the chalk, as is often supposed; unfortunately, however, his section gives them rather the appearance of being so. We should also notice that although, on the evidence of fragments of Ammonites and Belemnites, he prefers to refer the red beds of the Kentish Town section to the Lower Greensand, none of this formation is represented in the section as lying beneath this part of London.

This convenient little publication, so full of valuable and condensed information, for so small a sum, will be of such great use to the members of the numerous field clubs, that we fear it will soon be out of print. What are 500 copies among so many who would wish to have it?

Snioland; or, Iceland, its Jökulls and Fjalls. By William Lord Watts. (London: Longmans and Co., 1875.)

In a recent number (vol. xii. p. 333) we published a letter from Mr. Watts announcing the important fact that he had succeeded in crossing the Vatna Jökull. So far as is known, this is the first time that this jökull (which means "glacier," and is probably cognate with the latter part of our word *ic-icle*) has been crossed, and the fact is creditable to Mr. Watts's determination and perseverance. The little book before us contains a narrative of an unsuccessful attempt to accomplish the same object, made by Mr. Watts in the summer of 1874. We regret to have to say that the narrative is a disappointing one. It is in the form of a rough diary, which seems to have been sent to the press in its crude form and published with little or no revision. A large portion of the book is occupied with a statement of the many difficulties, petty and serious, which Mr. Watts and his party encountered in the journey from Reykjavik, by the Geysers, Hekla, and the Myrdals Jökull to the Vatna Jökull, and there is really very little information about the region through which he passed. The entire narrative is extremely vague and unsatisfactory, and if Mr. Watts has any literary faculty, he certainly does not show it here; the reading of his narrative is a heavy task. Mr. Watts ought to know a great deal about the region with which this narrative is concerned, and especially about the jökulls in the south of Iceland, and we would advise him to put this information into a systematic form, make but little reference to the difficulties he encountered, obtain a few photographs on a much larger scale than the insignificant things which appear in the present work, and we have no doubt he would make a substantial contribution to our knowledge of Iceland. The party succeeded in getting only about half across the Vatna Jökull, when, from want of the necessary means to go further, they were compelled to turn back, after Mr. Watts had rather unnecessarily and sensationally planted the union jack at his furthest point. Mr. Watts's carelessness, to put it mildly, extends even to his use of language. The use of "laid" for "lay" might possibly be justified by eminent precedents; "peninsular of rock" may be a misprint, but "pulverent" is unjustifiable, and "mollusc" is shocking.

Perhaps the most tangible piece of information conveyed by Mr. Watts is contained in the following paragraph:—

"To sum up, this hitherto untrodden Vatna Jökull is a mountainous tract, surmounted by a rolling plateau, containing numerous volcanoes, one or more of which, upon the north, appear to be in a state of pretty constant activity, while numerous others in all probability are paroxysmal, most likely exhibiting all the phenomena characteristic of (if I may be allowed the term) *bottled up volcanoes*. This tract, together with the Odatha-hraun,

and the centre of Iceland with its numerous mountains, is a new volume of Nature, the first leaf of which has only just been cut, but whose secluded fastnesses will amply repay investigation."

In an appendix Mr. Watts gives some information as to equipment, which intending travellers in Iceland will find useful. The map at the end is on too small a scale to be of much use.

The main object of Mr. Watts's narrative is to attract attention to Iceland and induce travellers to co-operate in its exploration. We hope the work will serve this laudable object, as there is no doubt Iceland presents a handy and fertile field for explorers. Mr. Watts himself deserves great credit for what he has already achieved; we hope he will continue his work, and in a future publication add something of permanent value to our knowledge of the interesting island.

Report on the Neilgherry Lorantheaceous Parasitical Plants destructive to Exotic Forest and Fruit Trees. By George Bidie, M.B., Madras. (Printed by E. Keys, at the Government Press, 1874.)

SURGEON-MAJOR BIDIE has in this volume presented to the Indian Government a report on the parasitical plants which prove destructive to forest and garden trees on the Neilgherries, and on the best mode of remedying the evil. The whole of these destructive parasites belong to one natural order, Lorantheaceæ, represented in this country by a single species, the Mistletoe, and to two genera, *Loranthus* and *Viscum*. The fruit of the Lorantheaceæ is characterised by the envelopment of the seed in a layer of a viscid substance, described by Dr. Bidie as intermediate in character between resin and india-rubber. Outside this viscid layer is a pulpy body which serves as food for birds and squirrels. After devouring this the seed is rejected, or, in the case of squirrels, passes unharmed through the body, and then adheres to the bark of any tree on which it may be cast. If the immediate conditions are unfavourable, the seed will be preserved in a state capable of germination for a very considerable time beneath its viscid covering. With regard to the mode of germination, Dr. Bidie has nothing to add to the information already furnished by Mr. Griffith and Dr. Hooker. With reference to the mode of attachment between the parasite and the host, the author states that although very firmly attached, there is no actual interlacing of the tissues; and that in some instances, after maceration in water for a few days, the parasite could be separated from the host without much difficulty. It is noteworthy that native Indian trees and shrubs do not appear to suffer nearly so much from the attacks of the Lorantheaceæ as introduced, especially Australian, species. One foreigner, however, which appeared quite exempt from their ravages, was the "blue gum," the *Eucalyptus globulus*, which has already so many other useful qualities placed to its credit. Dr. Bidie asserts that the Lorantheaceæ derive their nutriment not from the descending elaborated, but from the crude ascending sap of the host; hence their need for green foliage containing chlorophyll and possessing stomata, in which other parasites are deficient. The volume is embellished by fifteen large lithographs representing the different species, and illustrating the structure of the fruit and the mode of parasitism of the order.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Personal Equation in the Tabulation of Thermograms, &c.

MR. PLUMMER, in his letter (NATURE, vol. xii. p. 395), has missed the point of the review of the work of the Meteorological

Office referring to the tabulation of temperatures (vol. xii. p. 101). From 1,283 estimations of tenths of seconds, as tabulated by the highly-trained and experienced observers at Greenwich, he shows that the whole seconds estimated were 15 per cent. of the whole number, and thereupon remarks that this is precisely the excess of whole seconds that is taken in the review of the work of the Meteorological Office as indisputably proving the carelessness of the tabulations at the Kew Observatory. This is a mistake. Kew was not singled out for criticism because the whole degrees tabulated there amounted to 15 per cent. of the whole number, but because of "the irregularity of the tabulations, more especially as regards the tabulations from day to day." An examination of the tabulations at Kew from day to day shows that there are first-class tabulators in that Observatory, but it also shows there are others whose work is inferior. Thus, in the first published sheet for Kew, viz., January 1874, on seventeen of the days the whole degrees tabulated amounted on each of these days to at least 25 per cent., and the average of the whole seventeen days reached 31 per cent., or nearly a third of the whole. On the remaining fourteen days of the month the average was 14 per cent. Hence the variations of the numbers of whole degrees from month to month, which, as stated in the review, were 172 for January, 87 for February, 127 for March, and 94 for April. It is this irregularity in the work of tabulation which has lowered the character of the work done at Kew.

The averages calculated from 6,696 tabulations showed that the number of whole degrees read off at the seven observatories were 8.4 per cent. of the whole at Stonyhurst, 15.0 at Kew, 19.5 at Aberdeen, 21.2 at Armagh, 23.7 at Falmouth, 24.7 at Valencia, and 24.8 at Glasgow. So far as the mere average numbers are concerned, the tabulations at Stonyhurst and Kew are satisfactory; not so, however, is the work done at the other five observatories, especially the last three, where, on an average of 6,696 tabulations, a fourth part of all the numbers tabulated were whole degrees. For particular months the percentages are sometimes very large. Thus, at Aberdeen during January 1875, the following are the percentages of the different decimal places of the dry-bulb readings as printed by the Office:—

Decimal places.	'1	'2	'3	'4	'5	'6	'7	'8	'9	'0
Percentages.	11	6	5	7	6	6	6	5	9	39

From this examination it is seen that 50 per cent. of the whole readings are assigned to two of the decimal places, viz. '0 and '1, of which 39 per cent. are whole degrees. The largest percentages are not, as in the cases adduced by Mr. Plummer, distributed in different parts of the decimal scale, but stand together, viz., '9, '0, and '1. As regards the column for each particular hour, out of the thirty-one readings, nineteen whole degrees occur in the 5 A.M. column, eighteen in the 8 P.M. column, sixteen in the 5 P.M., fifteen in the 6 A.M., fourteen in the 4 P.M., thirteen in four of the columns, twelve in six, and so on, down to eight whole degrees in one column, and seven in another, than which no fewer whole degrees occur in any column. It is unnecessary to make any remark on these figures.

The Meteorological Office has published in their Quarterly Weather Reports the monthly extremes of temperature in two forms, viz. in figures, and in curves of temperature. These were compared and the results stated in the review, from which it was shown that as regards the first month's extremes, fourteen in number, there were twelve errors in the numbers as published by the Office; and as regards the first year's extremes, 168 in all, there occurred forty-one errors of temperature varying from 0°4 to 9°6, and twenty-two errors as regards the day and nine errors as regards the hour of occurrence. Altogether twenty-nine months have been examined with the general result of an average of fully four errors in stating each month's fourteen extreme temperatures. Now it is on the large proportion of errors made in stating the extreme temperatures (for the prevention of which one of the twenty-seven regulations for the Director of the Central Observatory was specially designed), taken in connection with such results as those given above for one of the observatories for January last, that the charge of inaccuracy in this very costly but vitally important part of the work of the Meteorological Office is based. This charge, Mr. Plummer's letter in no way meets. The simple course is to see that this department of the Meteorological Committee's work, including that of the outlying observatories, be brought under some sort of satisfactory control.

THE REVIEWER

Ocean Circulation

As the strength of Mr. Croll's conviction that he has completely demolished the "gravitation theory" of oceanic circulation by the "crucial test" to which he subjected it before the Geographical Section of the British Association, is not unlikely to influence the minds of some, I shall be glad to be allowed to point out (1) that I have never denied the existence of a horizontal "wind-circulation," and (2) that the doctrine to which he applied his test was not mine, but a creation of his own. For his whole argument was based on the assumption that the ocean is in a state of static equilibrium; whereas the theory I advocate, which was originally advanced by Lenz, and which Sir William Thomson (in commenting upon Mr. Croll's paper and my reply to it) pronounced to be a matter "not of argument, but of irrefragable demonstration," is, that the ocean never is and never can be in a state of equilibrium, so long as one part of it is subjected to polar cold, and another to equatorial heat; but that it is in a state of constant endeavour to recover the equilibrium which is as constantly being disturbed.

If the boiler and water-pipes of a heating apparatus be filled with water whose temperature is that of the building in which it is placed, the whole mass of fluid is in a state of equilibrium; but the lighting of the fire beneath the boiler disturbs that equilibrium, and produces a circulation, which will be maintained as long as the water is being alternately heated in the boiler and cooled by the atmosphere of the building.

Suppose that the elongated basin of the Mediterranean, instead of lying E. and W., were to be turned N. and S., so that its water, instead of being exposed (as at present) to a practical identity of thermal influences, should be subjected at one end to arctic cold and at the other to almost tropical heat: instead of remaining in its present state of nearly perfect equilibrium, it would have a circulation like that which I have exhibited in the trough-experiment.

The only objection raised by Mr. Croll which has even a show of validity, is based on the supposed "viscosity" of water, which he asserts to be sufficient to prevent the disturbance of thermal equilibrium from exerting the effect which the "gravitation theory" attributes to it. This assertion has now been completely disproved by the masterly investigations of Mr. Froude; who has demonstrated experimentally—what the "wave-line theory" of Stokes, Rankine, and Sir William Thomson had rendered probable—that in the resistance to the motion of a ship through the water, the viscosity of the water itself is so small an element that it may be practically thrown out, water behaving as a nearly "perfect fluid," except where it moves over solid surfaces. Mr. Froude (in conversation with me) not only sanctioned my conclusion that a constantly renewed disturbance of thermal equilibrium *must* produce an oceanic circulation, but mentioned as an instance of the very small difference of downward pressure necessary to sustain such a circulation, that he had ascertained by repeated observation at the mouths of harbours, lochs, and fiords, that wherever the water within has its salinity at all reduced by a mixture with fresh water, there is an underflow of sea-water setting inwards, precisely as in the Baltic and Black Sea Straits.

Mr. Croll attempted to draw a further disproof of the "gravitation theory" from the *Challenger* observations on the temperature of the upper strata of the Antarctic Sea, at and near the ice-border. These observations show that a stratum of water of from 32° to 29° overlies a stratum of from 34° to 32°; which is considered by Mr. Croll as a death-blow to my assumption that the coldest water sinks to the bottom. Now, since I have repeatedly pointed out that the water of melting field-ice, and *a fortiori* that of melting icebergs, will float on ordinary sea-water colder than itself, in virtue of its inferior salinity, and since Capt. Nares distinctly speaks of the cold surface-stratum as having this origin, it does seem to me not a little strange that Mr. Croll should have overlooked this consideration. It is obvious that, for the reason just stated, the descent of the cooled surface-stratum cannot take place in the polar summer at or near the margin of the ice: but that it takes place wherever and whenever the surface-cold is sufficient to check surface-liquefaction, and to cool down water of ordinary salinity to a temperature below that of the subjacent stratum, it will be hard for Mr. Croll to disprove.

I cannot but greatly regret that Mr. Croll abstains from subjecting his conclusions on this subject to the test of *personal discussion*. For if he would bring them (as I have brought my own) under the criticism of the Mathematicians and Physicists of Section A, he would find that, notwithstanding the